was the issue between my view of tuberculosis communicated from the cow and the view which Dr. Carpenter has been expounding, and I hope you will have room for the passage:—"The doctrine of a tuberculous virus was stated by Klebs in 1868, and has been advocated by him, as well as by Cohnheim, in recent writings. In its latest form this doctrine asserts the existence of a specific minute organism to whose agency the infection is due. The minute organism is called by Klebs *Monas tuberculosum*. The method of proof which I have followed in this work makes it impossible that the infective agency of a minute organism should in any way come into my view of the communication of bovine tuberculosis to man. I have rested the whole case upon certain minute identities of form and structure in the infected body, due to the mimicry of infection. Among other points there were the leaf-like and cord-like outgrowths of the pleura and peritoneum, these being the early stages of the lentil-like or pearl-like nodules and their connecting threads; the lymphatic glands, with distinct nodular formations in their substance; the lungs, with smooth-walled closed vomicæ or with encapsuled nodules. In the new formations generally there was a particular pattern of microscopic structure, in which giant-cells and epithelial-like cells figure largely, and there was a relatively high degree of vascularity. In all these points the disease in man is a mimicry of the parent disease in the bovine animal. That mimicry is not only in single features, but it is of the whole disease. It is possible to conceive of the juices and particles of the primarily diseased body acquiring a kind of spermatic virtue which gave them the power to communicate the specific disease as a whole and in all its several manifestations to another body in which they should happen to lodge. But it is hardly possible to think of a neutral living organism being charged with the power of conveying so complex details of form and structure from one body to another" ("Bovine Tuberculosis in Man," pp. 103, 4).

25, Savile Row, W., October 24 C. CREIGHTON

A Kinematical Theorem

PROF. MINCHIN'S Theorem in NATURE (vol. xxiv. p. 557) may be proved easily by considering the motion as due to the rolling of one closed curve on another back into its first position, their lengths being of course commensurable. If you measure y for the rolling curve from the straight line which forms the envelope, and x along that line, then the differential of the area between the envelope and the fixed curve is easily seen to be $ydx + \frac{1}{2}y^2dw$, where dw is the angle turned through by the rolling curve, and is equal to ds multiplied by the sum of the curvatures at the point of contact, which we shall call σ. The summation of the former part is a multiple of the area of the rolling curve, and therefore the same for all lines; that of the latter is half the moment of inertia of matter distributed over its perimeter with density σ, about the line in question. The result is therefore the well-known property of equi-momental ellipses. Similar reasoning, with the use of the property of the centre of inertia of a system, leads to the further result that when the perimeter of the envelope is of constant length, the line touches a circle, and different values of the constant correspond to concentric circles. In the same way by a property of the centre of inertia we may also prove immediately the known theorem that when the area traced out by a point is constant, the point lies on a circle, and different values of the constant correspond to concentric circles; and we may extend it to areas traced on a sphere.

JOSEPH LARMOR

54, Antrim Road, Belfast

IF Prof. Minchin will refer back to the *Bulletin des Sciences Mathématiques et Astronomiques* for August, 1878, he will, I think, find in a paper by M. Darboux the theorem stated by him under the above title in NATURE, vol. xxiv. p. 557.

C. LEUDESDORF

Pembroke College, Oxford, October 21

"The Dark Day"

REFERRING to the account of the phenomenon in New England on September 6 last (NATURE, vol. xxiv. p. 540), and in Mr. Harding's letter (p. 557), let me refer your readers to a succinct account of the occurrence on May 19, 1780, which they will find in Webster's Dictionary, "Explanatory and Pronouncing Vocabulary of the Noted Names of Fiction, &c." In *Public Opinion* (June 4, 1881) there is an account of a precisely similar occurrence on the morning of Sunday, November 8, 1819, known, it is remarked, as the "Phenomenon of 1819." The account of this phenomenon is very explicit, and the details furnished correspond so closely with the event of May 19, 1780, that a doubt might be felt whether there had been two such days, or whether there had not been some mistake made in regard to the date given. I wrote to *Public Opinion*, making inquiries (see *Public Opinion* of June 11, 1881, p. 755), but no reply has hitherto appeared to my inquiries. I may observe that the year 1819 would not coincide with any one of the sun-spot cycle of eleven years from 1780 to which the New York *Nation* refers.

A. TREVOR CRISPIN

6, Melbury Terrace, Harewood Square, N.W., October 22

OWENS COLLEGE SCIENCE AND LITERATURE FELLOWSHIPS

THE first award of these Fellowships, of the annual value of 100*l.* each, which are intended to encourage original investigation, was made on Friday last by the Council of the Owens College. These are remarkable as being the only fellowships given in any University or College in the United Kingdom solely for the encouragement of research. They are not awarded on the results of examination, but after consideration of documentary or other evidence. Every holder of a fellowship is expected to devote his time to the prosecution of some special study, and before the close of the year to give evidence of progress by the preparation of a thesis, the delivery of a lecture, or the completion of some research. He may also be called upon to render some service to the College either by acting as occasional examiner or by giving instruction by lectures or otherwise to the students.

Of the thirty candidates four gentlemen were elected to Fellowships. Of these one is awarded to Mr. Alfred Sidgwick, B.A. of Lincoln College, Oxford, in the Department of Logic; two were awarded in the Department of Chemistry, namely, one to Dr. Bohnslav Brauner, of the University of Prague, who has already published several papers on original subjects, some from the laboratory of the Owens College; and a second to Mr. Harry Baker, Dalton Chemical Scholar of the College, who has likewise published several papers in the *Journal* of the Chemical Society. These two gentlemen will continue their researches, devoting the whole of their time to original investigation. In the Department of Biology an award has been made to Mr. H. Marshall Ward, B.A., F.L.S., of Christ College, Cambridge, at one time a demonstrator in the Owens College, who has recently distinguished himself as Government cryptogamist in Ceylon, in an investigation of the cause of the coffee disease.

THE AGE OF THE IGNEOUS ROCKS OF ICELAND

DURING a recent visit to the south-west part of Iceland, one or two points connected with the general geological structure of the island came under my observation, which I do not remember to have seen noticed before, and which seem to me to be of sufficient interest to be put on record. It is well known that the rocks of the island are of very different ages, some going back to the Miocene period, while others are quite of yesterday's date. It is also perhaps a general belief that the volcanic forces may have continued to be more or less active from the time that the older Miocene basalts and tuffs were erupted down to our own day. I doubt very much whether there is any evidence to justify this conclusion, and will presently mention some of the facts which lead to a very strong suspicion that a prolonged period of repose supervened after the accumulation of the Miocene rocks, and before the eruption of the later lavas, &c., had begun. The Miocene group consists of a vast

series of basalt-rocks with interbedded layers of palagonitic tuff, &c. These rocks, so far as my observations go, exactly resemble those of the Færöe Islands. The basalt-rocks are chiefly anamesites, but some are true basalts, while others are dolerites. But in the areas traversed by me I saw none so coarse-grained or so highly porphyritic as those which occur so abundantly in Stromöe, Österöe, and other islands of the Færöes. They form lofty plateaux, deeply gashed with gorges, and abruptly truncated, so as to present bold cliffs and precipices to the low grounds at their base, as in the case of the Esja near Reykjavik. Moreover, they appear to be developed chiefly in the maritime districts. Only a glance at these basaltic masses is needed to convince one that they are the mere fragments of what must once have been a most extensive plateau. The Esja, built up chiefly of comparatively horizontal beds of basalt, tuff, &c., rises to a height of nearly 3000 feet above the low tracts at its base. Nor can there be any doubt that these beds formerly stretched far away in all directions, and that they have since been removed by the various agents of denudation from the broad undulating low grounds, over which they may still be traced, sometimes continuously for many miles, at other times in sporadic hills and rising grounds, which peer above the surface of the recent lavas by which they are surrounded. In short, the Miocene basalt-rocks of Iceland present precisely the same features as the similar rock-masses of the Færöes. Like the latter they probably formed at one time a wide elevated table-land, which has since been cut down and worn away—the lofty walls of the Esja, &c., serving to give us some idea of the enormous erosion that has taken place. Now all this vast erosion had been effected before any of the later lavas, agglomerates, tuffs, &c., in the south-west part of Iceland were erupted. In the region between Hafnarfjörd and Krísuvík the lavas have poured through old valleys in the Miocene rocks and spread themselves out over the highly denuded surface of the latter in the opener low grounds. In a word, it is evident that in the south-west part of Iceland a long interval separates the accumulation of the Miocene basalt-series from the eruption of the later volcanic rocks, and I incline to think that the same break in the continuity of volcanic action will be found to hold true for the rest of the island. I believe it will be found that there is no more connection between the display of volcanic activity in Miocene times and that of the present day in Iceland, than there appears to have been between the volcanic action which manifested itself in Scotland at such widely separated periods as those of the Lower Old Red Sandstone and the Carboniferous. Had there been more or less continuous volcanic activity in Iceland from Miocene times down to the present, we might well be surprised that the later volcanic masses are not much more considerable than they are. If we think of the time required for the removal by denudation of some 3000 feet of basalt-rocks, &c., over thousands of square miles, we must be prepared to admit that the volcanic forces cannot have been continuously active. Either they have not been so, or the denuding agents have far surpassed them in energy.

There is another point which interested me. I found that the whole of the south-west region had been glaciated before the eruption of the later volcanic series. The Miocene basalts are everywhere ice-worn and abraded; *roches moutonnées* are well-marked, and in many places glacial ruts and striæ are conspicuous. Glacial gravels and coarse boulder-clay are likewise sprinkled over the surface of the low-lying tracts. Between Reykjavik and Hafnarfjörd the glaciation is distinctly from south-east to north-west, and could not have been the result of any mere local glacier. The whole wide tract has been overflowed by a general *mer de glace*. And if this be the case with that part of Iceland which now enjoys the mildest climate, we may be sure that the rest of the island must likewise have been enveloped in ice during the Glacial period. In the south-west region all the traces of glaciation are strictly confined to the Miocene areas. Nothing of the kind is visible upon any of the later volcanic rocks. These last have flowed over a glaciated surface, for the ice-worn Miocene basalts terminate abruptly at the margins of the wide sheets of black scoriaceous lava, as do also the drift-accumulations of glacial gravels and erratics, while now and again ice-worn knolls of basalt-rock may be seen rising up like islands in the midst of the later lava-fields. Everywhere the lavas and their associated agglomerates and tuffs show their original surfaces—the only changes which they have undergone being the result of subaërial weathering. In a word, all the post-Miocene eruptions of the south-west are of later date than the Glacial period. It would be interesting to ascertain whether the same is the case throughout Iceland. As there is every probability that the great break in the continuity of volcanic action, of which I have spoken, is not confined to the south-west, but may hold true of the whole island, it seems not unlikely that the conclusions I have formed as to the post-Glacial age of the later volcanic series of the south-west will also be extended to the same series in other districts. In other words, we may yet be compelled to admit that the oldest eruptions of Hecla and her sisters are not only of vastly more recent age than any of the Miocene basalt-rocks, but belong to one of the latest epochs of which geology takes cognisance.

JAMES GEIKIE

THE EVOLUTION OF THE PALÆOZOIC VEGETATION

SOME statements made in Mr. Starkie Gardner's abstract (NATURE, vol. xxiv. p. 558) of the recent work of Saporta and Marion "On the Evolution of the Cryptogams" are so opposed to conclusions at which I have arrived that I can scarcely allow them to pass unchallenged, lest by doing so it may be inferred that I no longer oppose the French school of Carboniferous palæo-botanists on several vital points connected with the interpretation of the Carboniferous flora. But before doing so I may venture to suggest a doubt whether the time has yet arrived for making the attempt to trace the lines of descent of the Palæozoic flora. It is true that much has been done of late years to extend our knowledge of that flora, but perhaps at the same period our knowledge of the extent of our ignorance has, *pari passu*, been equally enlarged. We now possess accurate information respecting the structure of many well-known plants, but we have also obtained glimpses of the existence of many obscure but very important organisms which represent factors that cannot be left out of consideration in dealing with the problem of their evolution. Besides this, opinions of experts are widely divergent on some very important questions of interpretation affecting the relationship of conspicuous plants whose organisation is understood. So long as experienced palæontologists are disagreed on the relations of the Calamites to the Calamodendra, and of the Lepidodendra to the Sigillariæ, a scheme of evolution explaining the development of the Carboniferous flora can scarcely be possible. The French school of botanists still believe that what they call Calamites are Equisetaceous Cryptogams, whilst the Calamodendra are Gymnospermous Phanerogams. In like manner they believe the Lepidodendra to be Cryptogams, and as such to be devoid of all exogenous growths in the exterior of their stems, whilst they regard all the Lepidodendroid stems that possess such growths as Sigillariæ, and relegate them also to the Gymnospermic section of the vegetable kingdom. I am more than ever convinced that these views cannot be sustained, and I think that my memoirs on these subjects, especially Parts IX. and XI.,